Observations on Ants, Bees, and Wasps.—Part X. With a Description of a new Genus of Honey-ant. By Sir JOHN LUBBOCK, Bart., M.P., D.C.L., LL.D., F.R.S.

[Read November 2, 1882.]

(PLATE II.)

ON BEES.

Dr. Müller's Criticisms.—A recent number of 'Kosmos' contains a very courteous and complimentary notice, by Dr. H. Müller, of my recent book on Ants, Bees, and Wasps, which of course, coming from so high an authority, is especially gratifying. Dr. Müller, however, criticises some of the experiments by which I think I have shown experimentally that bees are attracted by different colors, and that they prefer blue to red, yellow, white, or green.

He remarks that in order to make the experiment absolutely correct, the seven glasses should have been arranged in every possible order, and that this would give no less than 5040 combinations. I did not, however, suppose that I had attained to mathematical accuracy, or shown the exact degree of preference; all I claimed to show was the order of preference; and I think that as in my experiments the position of the colors was continually being changed, the result in this respect would have been substantially the same.

Dr. Müller also observes that when a bee has been accustomed to come to one place for honey, she returns to it, and will tend to alight there whatever the color may be; and he shows, by the record of his own experiences, that this has a considerable influence. This is so. Of course, however, it applies mainly to bees which had been used for some time, and were accustomed to a particular spot. I was fully alive to this tendency of the bees, and neutralized it to a considerable extent, partly by frequently changing the bee, and partly by moving the glasses. While, however, I admit that it is a factor which has to be taken into consideration, I do not see that it is any argument against my conclusions. The tendency would be to weaken the effect of preference for any particular color, and to equalize the visits to all the glasses. This tendency on the part of the bees was, as my experiments show, overborne by the effect produced upon them by the color. So far from weakening my conclusions, the

fact, so far as it goes, tends to strengthen them, because it shows that notwithstanding this tendency the blue was preferred and the honey on colorless glass neglected. The legitimate conclusion to be drawn seems, I confess, to me, not that my mode of observation was faulty, but only that the preference of the bees for particular colors is really somewhat greater than the numbers would indicate.

Next, Dr. Müller objects that when disturbed from one drop of honey, the bees naturally would, and that in his experiments they actually did, fly to the next. He gives the two following cases in illustration:—

Blue.	Yellow.	Greenish yellow.	Scarlet.	White.	Red.	Green.	Violet.
	7	6	5	4	3	2	1

Violet.	Green.	Red.	White.	Scarlet.	Greenish yellow.	Yellow.	Blue.
8	7	6	5	4	3	2	1

in which, as will be seen, the bee came in both cases to the right-hand drop, and then went regularly along the line, whatever the color might be. Out of 240 cases he found that the bee, when disturbed, flew to the nearest drop in 207. As a matter of fact, however, this did not happen in my experiments, because, to avoid this source of error, when I removed the colour I gave the bee a good shake and so made her take a flight before settling down again.

According to my experience, bees differ considerably in character, or, I should rather perhaps say, in humor. Some are much shier and more restless than others. When disturbed from the first drop of honey, some are much longer before they settle on the next than others. Much also of course depends on how long the bee has been experimented on. Bees, like men, settle down to their work. Moreover it is no doubt true that, *cæteris paribus*, a bee in search of honey will go to the nearest source.

But, as a matter of fact, in my hundred experiments I only had a few cases like those quoted above from Dr. Müller. This arose partly from the fact that my bees were frequently changed, and partly because I took care, in removing the color, to startle the bee enough to make her take a little flight before alighting again. Dr. Müller says that in his experiments, when the bee did not go to the next honey, it was when he shook her off *too* vigorously. Under the circumstances, I should rather say that in the two observations quoted above he did not shake the bee off vigorously enough. The whole objection, however, is open to

the same remark as the last. The bee would have a tendency of course, like any one else, to go to the nearest honey. Hence I never supposed that the figures exactly indicate the degree of preference. The very fact, however, that there would naturally be a tendency to save themselves labor by going to the nearest honey, makes the contrast shown by my observations all the more striking. Dr. Müller's criticism does not in any way invalidate my conclusions, as he supposes, but, as it seems to me, strengthens my argument.

I have never alleged that it was possible in the case of bees (or for that matter of men either) to get any absolute and exact measure of preference for one color over another. It would be easy to suggest many considerations which would prevent this. For instance, something would probably depend on the kind of flower the bee had been in the habit of visiting. A bee which had been sucking daises might probably behave very differently from one which had been frequenting a blue flower.

So far, however, as the conclusions which I ventured to draw are concerned, I cannot see that they are in any way invalidated by the objections which Dr. Müller has urged.

I exhibit to the Society my original notes; and it will be seen that there is no such sequence as Dr. Müller supposes.

I am glad to see that Dr. Müller is himself about to make a series of experiments on bees with reference to color; and I doubt not they will be most interesting.

Hearing in Bees.—Aug. 27. I brought two sets of bees from different hives to two deposits of honey a few yards apart, and after arranging a telephone with a microphone in the circuit, disturbed one set of bees, holding a telephone close over the other, to see if they would make any noise which would affect the others. I tried it several times, but with no result.

I then placed one telephone just at the entrance of the hive; but whether the apparatus was connected or not, and whether the bees were disturbed or not, seemed to make no difference.

I then made the following experiment:—On the 30th September I put out a small quantity of honey on my lawn and brought some bees to it. I then set a musical box going, and continually replenished the honey and wound up the box. The weather was lovely, and all day a certain number of bees visited the honey.

Then on the 8th October I removed the honey to an open window

on the first floor, and set the musical box playing as usual by its side. I waited half an hour, but not a bee came. I need hardly say that the music was quite audible on the lawn. I then again put the musical box and the honey on the lawn, and the bees very soon again began work. After the lapse of an hour I brought the honey and musical box into the house, and placed them at an open drawing-room window less than 15 yards from where they had stood on the lawn. The music was kept going for an hour, but not a bee came.

The following day was again extremely fine. The bees came as usual to the honey. I let them feed till 10 A.M., when I removed the honey as before to the drawing-room. After the lapse of half an hour I set the box playing and waited half an hour, but not a bee came.

I then put the honey and musical box again out on a chair on the lawn 5 yards in front of the drawing-room window. The first bee found the honey in $5\frac{1}{4}$ minutes. I left it so for three quarters of an hour, and then brought the honey and the musical box into the house and put them just inside the window but out of sight. The box was kept playing for three quarters of an hour, during the whole of which a few bees kept hovering round the chair; but not a single bee found the honey, or even was attracted by the music into the room. I then took the honey and put it again on the chair outside. In less than 5 minutes nine bees had settled on it. I then brought it back into the room and put it, with the bees on it, where it had stood previously. The bees fed, returned to the hive, and came back again to the honey as usual, showing that they had not the slightest objection to enter the house.

I then took the honey and the musical box down to the hives. Immediately (*i. e.* about a yard) in front of my hives is a low wall; and I put the box and the honey on the far side of the wall, so that they were something less than 4 yards distant from the hive, but of course not directly visible. I then kept the music going for two hours, from 1.30 to 3.30 P.M., but not a bee came to the honey.

From these experiments we are, I think, justified in concluding either that the bees did not hear the music, or that, though they had been feeding close to the music, eight days was not a long enough period to suggest to them that there could possibly be any connexion between the honey and the musical box.

To decide between these two alternatives, I moved the musical box (without setting it to play) and honey to another part of the lawn about 15 yards from the first, and put an equal quantity of honey on a similar piece of glass at about the same distance both from the musical box and from the spot where the box had previously been. In half an hour there were several bees at the honey on the musical box, and none at the other. After this we had a week of rain. The next fine morning I again put out the musical box with some honey, and at a distance of about 15 yards a similar quantity of honey on a bit of glass on the grass. In half an hour there were several bees at the honey on the musical box, and none on the other.

I had intended to repeat this several times for greater security, but was unfortunately prevented by bad weather. The observations, however, indicate, as far as they go, that the bees did connect the presence of the musical box with that of the honey, and were guided by it, even if it were not playing, so long as they could see it, but that if they could not see it, even though it were playing, it did not assist them.

At first sight it might seem that these experiments are in direct opposition to the general idea, that a clanging noise is useful in causing bees when swarming to settle soon. This notion is as old as Aristotle, who says, "Bees also appear to have pleasure in noises, so that they say that they collect them into their hives by striking earthen vessels and making noises." He adds, however, "But it is very doubtful whether they hear or not."

The general opinion among writers on bees now seems to be that "tanging" is quite unecessary. Bevan says:—"A tinkling noise is generally, though erroneously, considered to be useful in inducing bees to settle; it is usually made by drumming smartly upon a frying-pan with a large key; and the cottagers call it 'tanging' or 'ringing.' It was probably practised, 'at first,' as Butler says, 'to proclaim to the neighbours that a swarm was up, serving as a public notification of the hive from whence it proceeded. This view of the matter is confirmed by the opinion prevalent in some districts, that unless the apiarian can prove the tanging, he cannot justly lay claim to the swarm if it happen to cluster on the premises of a neighbour. The original object of this proceeding, however, seems to be forgotten; and the practice is regarded by most of the cottagers as quite necessary to effect a speedy and satisfactory settling of the bees. Most

scientific apiarians discountenance it; and I am convinced that it is wholly useless.' Mr. Dunbar, during a period of nearly forty years, has only lost one swarm; and that, contrary to his own judgment, was most sonorously tanged"*.

I cannot from my own experience decide the point. Admitting, however, that a custom so ancient and so widely spread, is unlikely to be entirely without foundation, I would suggest as possible that what the bees hear under these circumstances are not the sounds which affect us, but the high upper tones near and beyond our range of hearing. Mr. G. Darwin and Lord Rayleigh, whom I have consulted on the subject, inform me that the presence of these inaudible overtones is unquestionable. Mr. Darwin says:—"The high overtones (generally non-harmonic overtones) are very strong within the limits of audibility; and it is almost certain, though not experimentally verified, that the overtones beyond the limits of audibility are strong also."

On Wasps.

Industry of Wasps.—The statement that wasps are as industrious as bees has been received with some incredulity, and has been by many regarded rather as a perhaps pardonable exaggeration arising from individual partiality, than as the strict and sober truth.

I thought therefore that it would be interesting to compare a wasp and a bee under similar circumstances for a whole day. Accordingly, on the 6th August I accustomed a wasp and three bees to come to some honey put out for them on two tables, one allotted to the wasp, the other to the bees. The last bee came at 7.15 P.M. The wasp continued working regularly till 7.47, coming at intervals of between 6 and 7 minutes. Next morning, when I went into my study a few minutes after 4 A.M., I found her already at the honey. The first bee came at 5.45; the second at 6.

The wasp occupied about a minute, or even less, in supplying herself with a load of honey, and made during the day, as shown below, no less than 116 visits to the honey, or 232 journeys between my room and her nest, during which she carried off rather more than 64 grains of honey. The bee sucked from 6 to 16

* 'The Honey-bee,' by Ed. Bevan, p. 91.

minutes, and made 29 visits to the honey. Next morning she made her first visit at 8.

As regards the wasp, I believe the record gives a fair idea of what an average wasp would do under similar circumstances. The bee, however, was a shy one; and, as shown in previous experiments, most bees would have come much more frequently. As regards the time she commenced and ceased work, however, she kept about the usual hours, and, as will be seen, began later and left off earlier than the wasp. It would, however, perhaps be unfair to the bees to regard this as indicating that they are less industrious than wasps. The difference may be due to their being more susceptible to cold.

Wasp.	Bee, L. W.	Wasp.	Bee, L. W.	Wasp.	Bee, L. W.
4.13		8.40		12.52	
4.32		8.45		12.56	
4.50		8.56		1. 4	1. 5
5. 5		9. 7		1.11	
5.15		9.14		1.20	
5.22		9.20		1.25	1.25
5.29		9.26		1.30	
5.36		9.37		1.35*	
5.43	5.45	9.43		1.43	1.40
5.50		9.50		1.48	
5.57	5.58	9.57		1.53	1.53
6. 5		10. 4		2	
6.14		10.10		2. 7	
6.23		10.15		2.12	
6.30	6.35	10.24		2.23	2.25
6.40		10.29		2.33	
6.48		10.37		2.39	2.41
6.56		10.45		2.45	2.45
7. 5	7. 4	10.50		2.55	
7.12		10.59		3. 2	3
7.18		11. 6		3. 9	3·16
7.25		11.15		3.17	
7.31		11.22		3.25	
7.40	7.41	11.30		3.30	3.32
7.46		11.35		3.37	
7.52		11.47	11.45	3.45	
8		11.55		3.55	
8.10	8.11	12. 6		4. 5	4
8.18		12.14	12.15	4.12	
8.24		12.22	12.25	4.19	4.16
8.29		12.36	12.37	4.28	
8.36	8.35	12.46		4.39	4.38

* She came and a strange one followed; they began to fight, and I separated them.

Wasp.	Bee, L. W.	Wasp.	Bee, L. W.	Wasp.	Bee, L. W.
4.46		5.50	5.50	6.55	
4.56		6. 5		7. 7	
5. 3	5	6.12		7.17	7.15
5.14		6.20		7.30	
5.25	5.20	6.30		7.36	
5.35		6.40		7.46	
5.46		6.46	6.50		

After this I was away from home for six days, returning on the 12th. During the interval I left the honey on the table, covered over, but with a small entrance for the wasp. On my return I found her still industriously at work. The following morning I got up at 3; but that morning she did not make her first visit till 5.10, returning at 5.19. No other wasp came to the honey.

On Ants.

Ants, Recognition by.—With reference to the interesting problem as to how ants recognize their nest companions, I mentioned in my last paper that I had tried the following experiment. took a few specimens of *Formica fusca* from two different nests, which I will call A and B, and placed them together. At first they were rather shy; but after awhile they fraternized. After they had lived amicably together for three months, I put two of these ants from nest A into nest B; but they were soon attacked vigorously and driven out of the nest.

I have now repeated and extended this test.

On the 16th June I put three specimens of *F. fusca* from my nest No. 81, with the same number from nest 71. Then on the 19th September, one of the six having died in the interval, I put the two from nest 81 into nest 71, and the three from nest 71 into 81. They were all attacked, though not very quickly or vigorously, but by the 21st September all five had been expelled.

Again, on the 25th September I took three ants from each of these nests and put the six together. Then on the 19th March following, one having again died, I put the two from 71 into 81 and the three from 81 into 71. They were all attacked; so that they were evidently recognized as strangers; but it seemed to me that the attack was less vigorous, and I could not be sure that they were either killed or driven out. In the course of the week three or four dead ants were brought out of each of the nests;

but I could not feel quite sure that they were those experimented with.

Lastly, on the 9th April I again put twelve ants, six from each of these nests, together, and kept them so till the 22nd October. I then took four of those from 71, put three into 81 and the fourth into 71. I also took four of those from 81, and put three into 71, and the fourth back into 81 among her old friends. The two ants thus restored respectively to their old nests were as usual recognized as friends and quite unmolested. As regards the other six, the results were as follows. The ants were introduced into the nests at 8.15 A.M.

	Nest 71.	Nest 81.
8.45.	One was being attacked.	One was being attacked.
9.15.	None were „ „	„ „ „
9.45.	Two were „ „	„ „ „
10.15.	One was „ „	„ „ „
10.45.	None were „ „	„ „ „
12.30.	Two were „ „	„ „ „
1.30.	Two were „ „	None were „ „
2.30.	One was „ „	„ „ „

I do not give these results as by any means proving that ants do not recognize their friends by means of smell. They do seem, however, to show that not even six months of close companionship under precisely similar conditions will so far assimilate the odour as to lead to confusion. If the recognition *is* due in any degree to this cause, the odour is therefore probably an hereditary characteristic.

The following little fact may be worth mentioning as bearing both on the power of communication possessed by ants, and also on their feeling towards a queen. I was starting a new nest of *Lasius flavus* in which were two queens. We allowed the ants to take one of them into their new glass house; the other we kept with a small retinue in a separate bottle. If this bottle is placed near the nest, some of the retinue leave it, go into the nest, and soon the ants come out in large numbers to see, I had almost said to pay their respects to, their queen.

Longevity of Ants.—In previous papers I have called attention to the considerable age attained by my ants; and I may perhaps be permitted to repeat here, *mutatis mutandis*, a paragraph from my last communication with reference to my most aged specimens, most of those mentioned last year being still alive. One

of my nests of *Formica fusca* was brought from the woods in December 1874; it then contained two queens, both of which are now still alive. I am disposed to think that some of the workers now in the nest were among those originally captured, the mortality after the first few weeks having been but small. This, of course, I cannot prove. The queens, however, are certainly more than eight, and probably now more than nine years old. In the nest of *Lasius niger* which I brought in on the 30th November 1875 there was no queen, and, as already mentioned, no workers have been produced. These two still living are therefore the original ones; and they must be between seven and eight years old *.

I had also some workers of *Lasius niger* which I began to observe on the 6th July 1875; the last of these died on June 15th, 1881.

Lastly, some of *Formica cinerea*, which I began to observe on the 29th November 1875, lived till the ants in this nest died off somewhat rapidly, the last on July 23, 1881. There were no queens in either of these nests; these workers therefore must have been more than six years old. The workers of *Lasius niger* above mentioned, which were born at latest in the spring of 1875, must now be more than seven, and my two queens of *F. fusca* more than eight years old.

Sensitiveness of Ants to the Ultra-violet Rays.—Mr. Wigner (of Messrs. Harland and Wigner) was good enough to prepare me a solution of iodine in bisulphide of carbon, and a second of indigo, carmine, and roseine mixed so as to produce the same tint. To our eyes the two were identical both in color and capacity; but of course the ultra-violet rays were cut off by the bisulphide-of-carbon solution, while they were, at least for the most part, transmitted by the other. I placed equal amounts in flat-sided glass bottles, so as to have the same depth of each liquid. I then laid them as in previous experiments over a nest of *Formica fusca*. In twenty observations the ants went seventeen times all under the iodine and bisulphide, twice under the solution of indigo and carmine, and once there were some under each. These observations therefore show that the solutions, though apparently identical to us, appeared to the ants very different, and that, as

* They are now dead, the last on the 25th of February. The two queens of *F. fusca* are (March 1883) still alive.

before, they preferred to rest under the liquid which intercepted the ultra-violet rays. In two or three cases only they went under the other bottle; but I ought to add that my observations were made in winter, when the ants were rather sluggish. I am disposed to think that in summer perhaps these exceptional cases would not have occurred.

Discovery of Ponera *in Britain.*—Lastly, I have to record the discovery by my daughters' governess, Miss Wendland (whose assistance I have already had occasion to acknowledge), of a nest of *Ponera contracta*, the first, I believe, ever found in this country. By an unfortunate accident a community of *Lasius flavus* obtained access to and destroyed them before I was able to make any observations on them. Their nest was under a stone; and the community consisted of about 20 individuals.

Description of a New Honey-Ant.

Mr. Bagot has been good enough to send me from Australia another species of Honey-ant, which appears to form the type of a new genus, which I proposed to myself to dedicate to M. Forel and to its discoverer. I am, however, unfortunately anticipated in the use of *Forelia* as a genus, it recently having been appropriated by Dr. G. Haller for a new form of Swiss Hydrachnidæ (see Mitt. naturforsch. Gesell. Bern, 1882, p. 18). In the preparation of the description, which I subjoin, M. Forel has been good enough to give me his valuable assistance.

Melophorus, gen. nov. (Plate II. figs. 1–10.)

Operaria inflata.—Long. 18 mill. Caput rectangulare, latius quam longius. Mandibulæ angustæ, striatæ, quinquedentatæ, dentibus irregularibus, dente anteriore longissimo. Clypeus brevis, subcarinatus, margine antico angulato, ciliato. Fovea clypei magna. Area frontalis triangularis, lata. Ocelli minuti. Oculi ad partem tertiam posteriorem capitis longitudinis (antice) siti. Sulcus magnus inter pronotum et metanotum. Metathorax late constrictus. Petioli squama verticalis, modice incrassata, antice convexa, postice plana, margine superiore emarginato. Subtiliter coriacea, rugulosa, subnitida; fronte inter laminas longitudinaliter rugulosa; abdomine nitido subtilissime transversim ruguloso. Sparsissime pilosa, capite infra setis longis barbato. Pubescentia corporis sparsissima, vix ulla. Femora subadpresse breviter pilosa. Tibiæ setis brevibus crassis spiniformibus. Testaceo-rufescens, abdomen testaceum, mandibulæ rufo-castaneæ, apice castaneo-nigro.

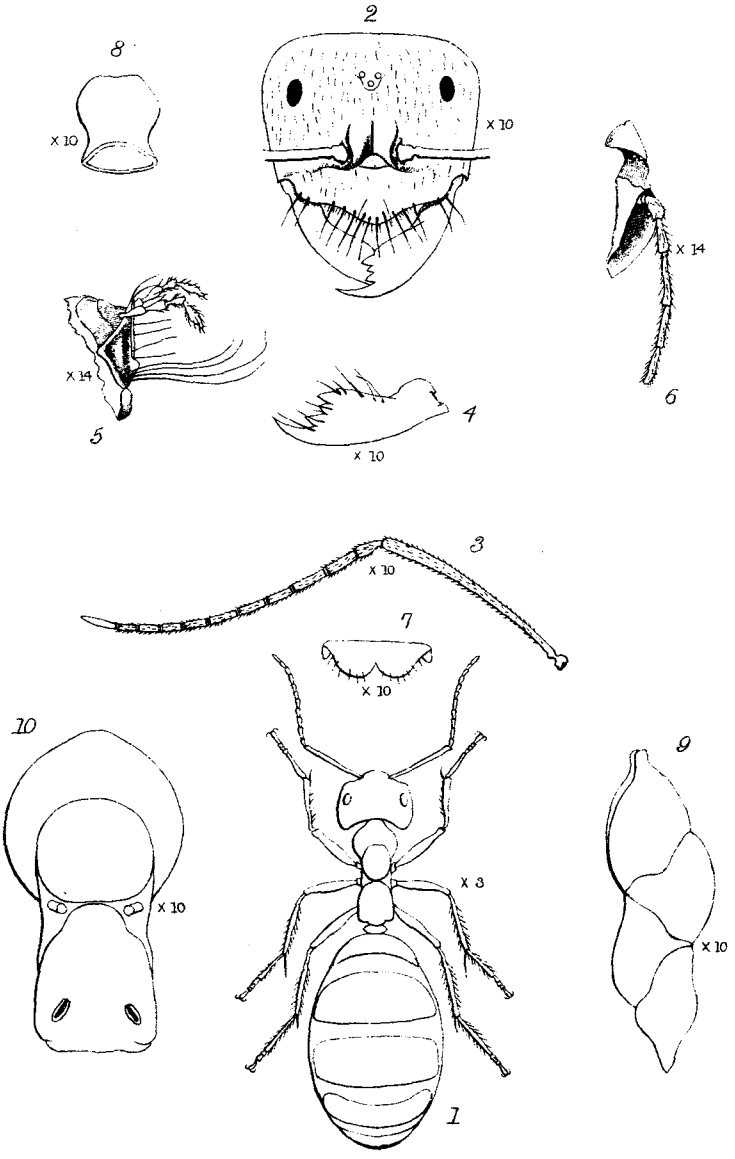
Habitat Australiam; lat. 21° S.

MELOPHORUS BAGOTI, sp. n.

OPERARIA.—Antennæ 12-articulatæ, articulis basalibus longioribus, apicalibus (ultimo excepto) brevioribus; palpi maxillares sexarticulati, labiales quadriarticulati; antennæ oriuntur ab angulis clypei posticis; fovea antennalis cum fovea clypei partim confluens; area frontalis lata; laminæ frontales distantes, breves, rectæ; clypeus brevis, antice ciliatus; mandibulæ angustæ, dentatæ; ocelli tres; metathorax constrictus; petioli squama verticalis; abdomen orificio cloacæ rotundo, ciliato, apicali; vesicula venenifica cum pulvinari; gigeriorum lamellæ breves, a basi fortiter divergentes.

DESCRIPTION OF PLATE II.

Fig. 1. Outline figure of a slightly swollen specimen of the new Australian Honey-ant, *Melophorus bagoti*, three times natural size.
2. The head, seen from above, ×10.
3. Antenna, ×10.
4. Mandible, ×10.
5. Labium, ×14.
6. Maxilla, ×14.
7. Labrum, ×10.
8. Knot, seen from behind, ×10.
9. An outline of the thorax, viewed laterally, ×10.
10. An outline of the thorax, seen from above, ×10.



E. Wilson del.
G. Jarman sc.

NEW GENUS OF HONEY-ANT.
(MELOPHORUS).

Hanhart imp.